

## SOLAR VARIATIONS

By H. H. CLAYTON

In the series of articles by Professor Marvin, Doctor Kimball, and Mr. Clough on the subject of solar variations in the MONTHLY WEATHER REVIEW for July and August, 1925 (vol. 53, Nos. 7 and 8), these authors analyze the work of the Smithsonian Astrophysical Observatory in regard to short period variations in solar radiation, and arrive at very discouraging views concerning the reality of these variations.

Professor Marvin says, "Unusual methods of analysis are required, because the total variation due to all causes is so small that it is entirely plausible that all of it may be nothing but errors of measurement." Doctor Kimball says, "Of the extreme range in values, which at Harqua Hala is from 1.958 to 1.871, or 0.088 gr. cal., and at Montezuma from 1.954 to 1.877 or 0.077 gr. cal., not over 0.003 gr. cal. can be attributed to some such common cause as solar variability, an amount which is quite negligible." And Mr. Clough says, "All these results point to the conclusion that if one could but wholly remove the atmosphere with all its depletions and fluctuations of intensity of transmission, and all instrumental errors, scarcely any variations of radiation intensity would remain."

Professor Marvin's article is an analysis by statistical methods of the final values of the derived solar constant published by Doctor Abbot and his collaborators. The only proper person to reply to this analysis is Doctor Abbot himself, who knows the details of his methods and his mechanism better than anyone else. In so far as my own work is concerned, I do not consider it necessary to enter into these details. No practical builder, when considering the construction of a house, would deem it essential to study botany and learn how boards grow, nor to study the details of nail-making machines before using the nails. All that he would think necessary would be to test the boards and nails in order to ascertain if they were suitable for his purpose. For such reasons I have not thought it essential to analyze Doctor Abbot's methods in detail before using his results, but rather to test these results by accepted methods and if they pass these tests to feel assured that the method of obtaining them was correct. In fact, when in 1915 I began using the measurements of solar radiation made by the Smithsonian Astrophysical Observatory, Doctor Abbot had already submitted these measurements to three crucial tests. In 1912 he established an observatory in the arid desert of northern Africa, and compared the results obtained there with those obtained simultaneously at Mount Wilson. There were large individual differences, but when the observations were massed in a single plot, it was evident that something in common was being measured at the two stations.

The next test was a comparison of the solar radiation measurements at Mount Wilson with changes in the contrast of brightness between the outer rim and the central area of the sun. For the year 1913, which was the year first used in my investigations, the correlation between the solar radiation values and the contrast of brightness between the central area and the outer rim of the sun was  $0.60 \pm 0.067$ .

A third test consisted in comparing the ratio between the intensity of the short-wave radiation and the long-wave radiation for different measured values of the total radiation. It is well known that when a body increases in temperature the proportion of short-wave radiation increases, so that the body becomes first red, then yellow, and

finally blue as the temperature rises. So Doctor Abbot found that as the sun's radiation increased the proportion of short waves increased.

These three tests seemed to me convincing evidence of the reality of the solar radiation changes, and I prepared a fourth test, namely, to see if the measured solar changes were correlated in any way with atmospheric changes on the earth. A comparison was made of the solar-radiation measurements with the temperature and pressure at stations scattered all over the earth. The results showed systematic changes which seem difficult to explain on any other grounds than a real relation. When the sun was hotter, the pressure fell and the temperature rose slightly in the equatorial belt and in high northern latitudes, and changed in the opposite way in intermediate latitudes. At some of the stations the correlation was as high as 0.50 and about four times the probable error.

In 1918 a solar observatory was established at Calama, in the desert of northern Chile, and simultaneous observations were made with Mount Wilson during the months June to October, from 1918 to 1920.

In order to present the relation between these simultaneous measurements in another way, I arranged the observations at Calama in a series of steps separated by 0.010 calorie, as shown in Table I, and for each class at Calama counted the frequency with which the simultaneous values occurred in different classes at Mount Wilson. It is evident that if there were no relation the observations would be scattered through the different classes at random, while if there were a relation between the two the observations would group themselves, and this grouping would be displaced systematically.

TABLE I.—Comparison of solar radiation measurements at Calama, Chile, and Mount Wilson, Calif., years 1918–1920

Values at Calama	1.920-9	1.930-9	1.940-9	1.950-9	1.960-9
Simultaneous values at Mount Wilson, number—					
1.900-9.....	1	1	0	0	0
1.910-9.....	(4)	1	1	2	0
1.920-9.....	3	3	1	2	1
1.930-9.....	0	(4)	(8)	3	0
1.940-9.....	1	1	5	(8)	1
1.950-9.....	1	4	2	7	(3)
1.960-9.....	1	0	1	6	3
1.970-9.....	1	0	1	0	1

This table brings out clearly that as the solar radiation values increased from grade 1.920-9 to grade 1.960-9 at Calama there was a maximum frequency of observations near the same grades at Mount Wilson and a steady progress from low to high values. I can not see what other interpretation can be put on this relation except that the observers were measuring the same phenomenon at the two stations, and that this phenomenon showed a range from grade 1.920-9 to 1.960-9, or more than 2 per cent, of the mean solar radiation value. There was no appreciable secular change during this interval, so that the whole of this variation must have been due to short period changes. According to Doctor Abbot's computations, these simultaneous observations show a correlation of  $0.49 \pm 0.05$ .

The scatter of the observations on each side of the maximum frequency is a measure of the errors of observa-

tion, and the fact that the maxima at Mount Wilson tended to come at a slightly lower level shows that there was some constant difference in level between the two, due to calibration of the instruments or to other causes.

In order to determine the probable error of the observations, I obtained all the differences between the pairs of simultaneous observations at the two stations, 110 in all, and found that they were distributed as shown in Table II.

TABLE II.—Distribution of the differences in solar radiation values observed simultaneously at Calama and Mount Wilson

Mean difference, thousandths of calories	-70	-60	-50	-40	-30	-20	-10	0	10	20	30	40	50	60	70
Frequencies.....	1	1	0	3	4	13	23	28	14	6	4	4	6	0	3

In counting the number of observations for -10, for example, all the observations between -6 and -14 were used; for zero, all the observations between -5 and +4 were included; and for +10, all observations between +5 and +14 were taken; and so on for each grade. As the distribution of these numbers evidently follows the normal law of distribution of errors of observations, they were reduced to percentages, and a curve of best fit was drawn through them. This can be done with much accuracy by means of the "Arithmetical Probability Paper," in which the probability integral is expanded so that the plotted numbers fall along a straight line. These percentages and the normal curve of best fit are plotted in Figure 1.

From this curve the probable error of the differences is found to be  $\pm 0.0121$  calorie, and since this value is made up of the combined errors of observation at Mount Wilson and Calama, the probable error at one station is  $\frac{0.0121}{\sqrt{2}}$ ,

which gives a value of  $\pm 0.0086$  for the observations at one station, assuming the errors at the stations to be equal. Or if we assume, as is probable, that they were slightly larger at Mount Wilson, we may take the probable error there as  $\pm 0.009$ . There are other ways of determining the probable error, but I regard this method as the most accurate one. It implies that the observations are independent of each other. Any correction which served to reduce the means of the observations to the same level or the same zero on the scale of measurements used would not alter the results in a way to change the conclusion drawn from them.

I used the observations at Mount Wilson for a study of the correlation between solar radiation and temperature and pressure in Argentina and we are now in a position to determine how much error was involved. In one comparison, I took all the highest values of solar radiation between the years 1909 and 1918, and determined the averages of the solar radiation values for each of the 30 days following the high solar radiation values, and for five days preceding. The number of cases varied somewhat, but averaged about 35. I then obtained averages of the temperature for each of the corresponding days at Buenos Aires. These means, after allowing an interval of three days for a lag in the effect, showed a correlation of 0.66 with the means of solar radiation. The mean values of solar radiation ranged from 1.930 to 1.952, while the probable error of the differences between the pairs of observations is

$$\pm \frac{0.0121}{\sqrt{35}} = \pm 0.021.$$

The observed range is hence 10 times the probable error of the means. This, it seems to me, is sufficient evidence that I was dealing with real solar changes and that the correlation is a real one.

In another case, I correlated 10-day means of solar radiation with 10-day means of temperature at various stations in Argentina, and obtained correlations exceeding 0.80 between the mean temperatures and the mean solar radiation values. The range in the mean values of solar radiation in this case is 0.032 gram calorie, and the probable error is

$$\pm \frac{0.0121}{\sqrt{10}} = 0.0037.$$

Here the observed range in mean values is about nine times the estimated probable error of the pairs of values; but this is somewhat too great, because there were some gaps in the solar radiation observations. Allowing for these, the range is about seven times the probable error of the means. Here again it is evident that I was dealing with real solar changes and that the correlations are real.

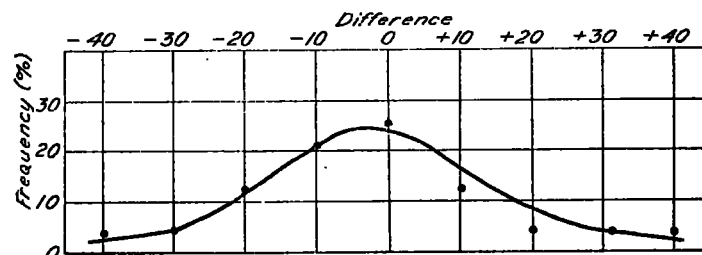


FIG. 1.—Distribution of the differences between the values of solar radiation measured simultaneously at Calama and Mount Wilson (unit .001 gram calorie per minute)

Turning to the more recent measurements at Montezuma, in northern Chile, and Harqua Hala, in Arizona, for the interval April 1922 to November 1924, I have counted from the dot diagram of Doctor Kimball in the July, 1925, MONTHLY WEATHER REVIEW, the number of observations in each grade at Harqua Hala between the successive values at Montezuma of 1.890, 1.900, 1.910, etc. The results are shown in Table III.

TABLE III.—Comparison of the numbers of simultaneous solar radiation measurements in each grade, at Montezuma and Harqua Hala, years 1922-1924

Values at Montezuma.....	1.890-9	1.900-9	1.910-9	1.920-9	1.930-9
Values at Harqua Hala:					
1.890-9.....	1				
1.890-9.....	(3)	4		6	1
1.900-9.....	0	4	11	11	2
1.910-9.....	2	7	(20)	21	10
1.920-9.....	1	(11)	19	(24)	19
1.930-9.....	2	4	10	10	(23)
1.940-9.....		1	5	6	11
1.950-9.....				1	3

Here again we see that when observations were made in one grade at Montezuma there was a maximum frequency in the same grade at Harqua Hala. The only exception to this is in grade 1.900-9. I can see but one explanation of this progressive sequence in the maximum frequencies at Harqua Hala with increasing values at Montezuma, and that is that both observers were measuring solar radiation values which progressed from grade 1.890-9 to grade 1.930-9. This range is over 2 per cent of the mean solar value. If we drop grades 1.890-9 and 1.900-9, there still remains a progressive change from grade 1.910-9 to grade 1.930-9 of over 1

per cent. Yet for this same interval Doctor Kimball's analysis leads him to assert that not over 0.003 gram calorie can be attributed to some common cause, as solar radiation.

If we take all the simultaneous observations represented in Doctor Kimball's diagrams for the interval from October, 1920, to November, 1924, the simultaneous occurrence of the maximum frequency in the same grades from grade 1.910-9 to grade 1.950-9 is very clear. These frequencies for the different grades are shown in Table IV.

TABLE IV.—Comparison of the numbers of simultaneous solar radiation measurements in each grade at Montezuma and Harqua Hala, years 1920-1924

Values at Montezuma	1.910-9	1.920-9	1.930-9	1.940-9	1.950-9
Values at Harqua Hala:					
1.880-9		2	2		
1.890-9		6	1		
1.900-9		11	2		
1.910-9	(21)	21	11		
1.920-9	19	(37)	20	5	
1.930-9	10	23	(39)	4	
1.940-9	6	6	14	(11)	7
1.950-9		1	9	7	(8)
1.960-9			3	3	2
1.970-9					3

The results indicate clearly that the observers at Montezuma and Harqua Hala were shooting at the same target and that the change from grade 1.910-9 to grade 1.950-9 is a real solar change. The scatter of the observations on each side of the maximum frequency in each grade gives a measure of the errors of observations.

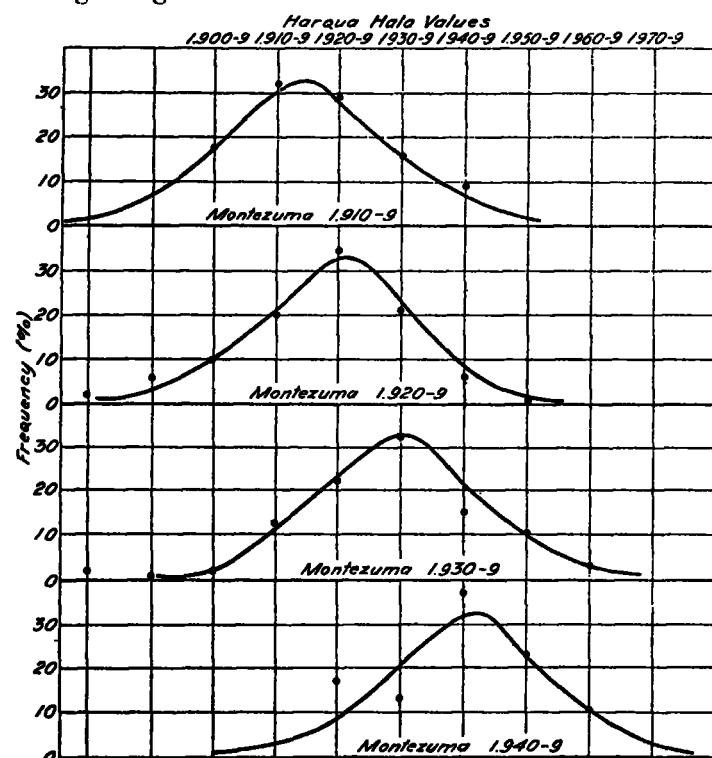


FIG. 2.—Frequency of difference of solar radiation at Harqua Hala, corresponding to simultaneous observations at Montezuma, 1920-1924, taken in grades differing by .010 calorie

The observations are sufficiently numerous in the four grades from 1.910-9 to 1.940-9 to permit an estimate of the probable error of the measurements in each grade. The frequencies were reduced to percentages and plotted on straight line probability paper. From these plots the probable error of the measurements in each grade

was determined as follows: Grade 1.910-9,  $\pm 0.0082$ ; grade 1.920-9,  $\pm 0.0080$ ; grade 1.930-9,  $\pm 0.0093$ ; and grade 1.940-9,  $\pm 0.0087$ . These probable errors are for the combined errors at the two stations. In order to obtain the probable error of the individual measurements at a single station, it is necessary to divide by  $\sqrt{2}$ . This gives a minimum value of  $\pm 0.0057$  for grade 1.920-9 and a maximum value of  $\pm 0.0066$  for grade 1.930-9. These four independent determinations are thus very accordant in showing a range of 0.040 gram calorie in the solar radiation, and a probable error of  $\pm 0.006$  gram calorie in the individual measurements. A plot of the percentages in each grade and of the curves of best fit are shown in Figure 2.

The question arises, Why did Doctor Kimball arrive at such a different result? There are, I think, two reasons. In the first place, Doctor Kimball broke up the period of observation into three parts, reducing the range of the solar variations, while the errors of measurement remained constant. To any one who has studied the method of correlation, it is evident that correlation coefficients are greatly reduced by such a proceeding, and may be reduced to almost zero by restraining the range of the phenomena measured within sufficiently narrow limits. Secondly, Doctor Kimball assumes that a small correlation coefficient proves that there is no relation between the phenomena compared. This assumption is, I think, erroneous, although it is frequently made.

Some years ago a case was brought to my attention, where the run-off of a river showed no correlation with the rainfall in the watershed of the river. By the methods of computation used by Professor Kimball it might have been shown that there was an insignificant relation between the two. This seemed to me an improbable conclusion, and when I looked into the matter I found that the river was fed by two nearly equal branches, in one of which there was a relatively steep descent and the water from the rainfall was fed quickly into the main stream, while in the other branch it was fed more slowly, so that the two flows tended to neutralize each other and produced the result mentioned. The whole of the variations in the main river was undoubtedly due to the rainfall, and yet by the usual methods of treating two results, they showed no correlation. The point I wish to bring out is that high correlations undoubtedly show close relation between phenomena, but low correlation does not prove that there is no intimate connection between the two, as many persons assume.

In addition to the evidences of solar variability which I have already recited, I have found that in the average of 200 cases that there is a sharp maximum of solar radiation coinciding with the times of maxima of faculae on the sun,\* using for this study the observations published by the Greenwich Observatory. For the months of April to September there were 121 observed cases and the mean maximum value varied from the mean values of preceding and following days to the extent of nine times the probable error of the mean.

I found also that there was a marked depression of solar radiation when sunspots and their attendant faculae crossed the central area of the sun. In this case the depression of the mean solar radiation below the mean of the values obtained when the spots were near the limb of the sun was seven times the probable error of the means.

\*See Smithsonian Miscellaneous Collections, vol. 77, No. 6.

Fowle has recently shown that there is a similar relation to the position of flocculi.

Further evidence of solar variability is furnished by the high correlations I have found between solar radiation values and certain atmospheric conditions in various parts of the world, which are published in the Smithsonian Miscellaneous Collections and elsewhere. Similar evidence is furnished by the work of others. From seven-day means of the pressure differences between Christiania and Bergen and of solar radiation, Helland-Hansen and Nansen found a correlation coefficient of 0.63 between the two for the period from June 8 to September 6, 1915.

These correlations are too numerous and too high to be dismissed as mere chance. It is true that other comparisons, even at the same places, give small correlation between the solar radiation and meteorological conditions, but as explained above, small correlations do not necessarily imply no relation, and do not offset large correlations. There are many reasons for the smaller correlations. The first is the inaccuracy of the solar measurements. The measurements may be fairly exact for a while under favorable observing conditions, and then be exact under less favorable conditions. In such a case high correlation with atmospheric changes might be followed by low correlations. Again, it is evident that waves of atmospheric change set up by solar changes travel from place to place and are superposed on waves of changes originating in other centers. After a period of solar quiet an increased activity might show a high correlation with certain regions, to be followed later by a low correlation as waves of change came in from other regions, and yet the whole effect be due to changes in solar radiation. In such cases low correlation does not disprove solar relations.

But the evidence of correlation of changes at individual stations with solar changes is not the whole story; there is evidence of an orderliness in the relations over the world as a whole, which is utterly inconsistent with chance agreement. In the equatorial region and especially in the region extending from the Amazon across the Atlantic, Africa, the Indian and the western part of the Pacific Oceans, a fall of pressure accompanies increased solar activity whether we take years of increased radiation, months having high average values, or individual days of large values, and obtain an average. Furthermore, the pressure increases in intermediate latitudes, and finally this belt of increased pressure sways back and forth toward and away from the Equator in unison with the increase and decrease of solar radiation. This swaying toward and away from the Equator is clearly shown in the United States from the average of a large number of individual days of high, medium, and low solar radiation as well as from monthly and annual means.

There are two points in Professor Marvin's paper to which I feel I should reply. Professor Marvin has long felt that a change in solar radiation of one per cent or less is too small to produce any appreciable meteorological change. In considering questions like this one, it should be borne in mind that large and small have no absolute value, but are merely comparative. A thing may be very small from one point of view and very large from another. A tidal wave, for example, might be extremely small as compared to the great Atlantic, not having a breadth, let us say, of more than one-tenthousandth part of the width of the great ocean, nor a vertical height of more than one thousandth part of the depth of the ocean, and yet it might have the power to wipe out or seriously damage every city along the Atlantic

coast, and thus be of the greatest importance in human affairs. It seems to me that the only way one can determine whether an observed change is important or not is to test it by comparison with the facts of nature.

That the range of the measured fluctuations of solar radiation should decrease with increasing refinement of the measurements is only what should be expected and by no means warrants a conclusion that the solar variations will cease with further refinements in the measurements.

The second point is in regard to the annual period in the solar radiation values claimed by Professor Marvin. If there exists a small annual period in solar radiation measurements, it would not materially affect the results of my investigation, because in investigating the relation between solar and meteorological changes I have in general separated the seasons and compared only the observations made at the same season.

The analysis of the methods of measuring solar radiation by Professor Marvin, and the interesting discovery by Mr. Clough of a correlation between the atmospheric transmission coefficient and the value of the solar radiation, may lead to an improvement in technique if their results are accepted, but they can not be held to prove that the variations of solar radiation are so small as to be negligible, unless all of nearly a dozen independent methods of checking and correlating the solar variability with other phenomena are false. In the reductions of the solar values by the methods in use at present, Doctor Abbot and his colleagues determine the amount of water vapor in the air by measuring the depth of absorption in one of the water vapor bands in the spectrum. It may be that these measurements were not of sufficient accuracy to eliminate the total effect of the water vapor. If so, that would explain the fact claimed by Mr. Clough that the solar radiation values changed in unison with changes in the water vapor content and the transparency of the air, and also explain the annual period in the intensity claimed by Professor Marvin.

#### SUMMARY

My conclusions are (1) That the comparison of simultaneous observations taken at Mount Wilson and Calama and later at Montezuma and Harqua Hala show that the derived values of solar radiation varied repeatedly between the values 1.910 and 1.960, and that the extreme range must have been considerably greater. A part of this change was due to a long period secular change, but the larger part was due to short period changes, especially during the interval July, 1918 to September, 1920.

(2) That the probable error of the individual observations at Mount Wilson is about  $\pm 0.009$  gram calorie, and at Montezuma less than  $\pm 0.006$  gram calorie.

For groups of values such as I used in my investigations the mean errors were equal to  $\frac{e}{\sqrt{n}}$ , in which  $e$  represents the probable error of the individual observation and  $n$  is the number of observations in the groups.

#### DISCUSSION OF THE FOREGOING PAPERS

By C. F. MARVIN, H. H. KIMBALL and E. W. WOOLARD

In the foregoing paper by Mr. Clayton, he declines, perhaps naturally, to discuss the analyses of Smithsonian solar constant values by members of the Weather Bureau staff, as published in this REVIEW for July and August, 1925. Since he holds that his researches are not concerned with the solar constant determinations except

as tools to be used in the work, he believes that Doctor Abbot is the one to deal with the bureau's findings. His paper is therefore in fact simply a restatement of views published in his earlier papers, especially "Solar Radiation and the Weather," in *Smiths. Miscell. Coll.* vol. 77, no. 6.

The question raised by the Weather Bureau has always been, in effect, are Doctor Abbot's remarkably refined determinations of the solar constant proper tools with which to forge a weather forecast? Our analysis has convinced us they are not. Mr. Clayton believes they are.

Doctor Abbot's annual report for 1925 as director of the Astrophysical Observatory, recently issued, contains statements which seem to remove at least the fundamental differences of view concerning the determinations which form the subject of this whole question. On page 103 he says:

The investigations hitherto made having indicated that a higher degree of accuracy in our solar measurements is needed to supply proper data for forecasting purposes, a very great deal of attention has been given to the elimination of small sources of error in the observations and reductions of solar radiation. Already the average deviation of individual days' results between Chile and Arizona is but one-half per cent. It follows that in order to obtain higher accuracy we shall be obliged to regard sources of error which formerly we supposed would always be negligible.

It seems that this statement by Doctor Abbot essentially confirms the general correctness of the results brought to light by our investigations of the derived values of the solar constant as published by the Astrophysical Observatory. Our view has been, and is, simply that the day-to-day variation of these values, due to all causes, as derived from the very best observations, became smaller and smaller as improvements of methods and places of observations were effected. Clearly, if the sun were the chief cause of the fluctuations, improving the methods would reduce the fluctuations only slightly. We find that the total variation due to all causes is of an order of magnitude of one-half of 1 per cent and less, according to the observations selected.

Now, it will be noticed that Doctor Abbot places the average deviation between Chile and Arizona at but one-half of 1 per cent. Our one-half of 1 per cent includes not only all errors, both instrumental and atmospheric, but also the real solar changes if there are any. Doctor Abbot's one-half of 1 per cent excludes solar changes and depends solely on the errors at the two stations. It can be shown that the significance of the close agreement of our results arrived at by two different methods is not lessened in any material degree by the fact that slightly different statistical units for measuring variability were employed in the two cases.

In my article in the *REVIEW* for July, page 303, Table 7, it was indicated that so far as it could be mathematically determined from observations available, the part of the total variation that might be ascribed to other than terrestrial origin, again from the best observations, ranged between 0.15 and 0.30 per cent. From the paragraph quoted above, I do not understand that Doctor Abbot differs essentially from these findings. In other words, the magnitude of the possible short-time variability of solar radiation considered as a whole is, as shown by the best observations, of the order of one-fourth of 1 per cent.

Can one-fourth of 1 per cent of variability be a safe basis of forecasting the weather for short or long periods in advance? It is universally agreed that all aspects of weather are due solely to atmospheric interceptions, by conduction to the atmosphere and its absorption of in-

coming and outgoing solar and earth heat respectively. If there were no interception or absorption of solar heat of any kind, weather as we know it would not exist. Now, supposing the total output of radiation does fluctuate one-fourth of 1 per cent, does not Mr. Clayton seriously disregard the question of the quantitative sufficiency of cause and effect, when he holds, as he seems to, that our weather phenomena can be primarily caused by, or forecast on the basis of, so small a fluctuation?

In this connection, reverting again to the seemingly close accord between the Smithsonian Institution and the Weather Bureau as to the basic fact concerning the average amount of possible solar fluctuations, I wish to quote a passage from an article by Doctor Abbot in the *National Geographic Magazine* for January, 1926, beginning on page 111, as a statement of at least one view he and Mr. Clayton hold regarding the cause and effect relations of very small solar fluctuations and the weather:

The fact is we have discovered that the sun is a variable star. Mr. H. H. Clayton, the eminent American meteorologist who has been cooperating in the work, has proved that very distinct changes of barometer, temperature, and rainfall are caused by these changes and the intensity of sun rays. He even goes so far as to say that he more and more believes, as his studies progress and bring new facts to light, that all that we call weather—symbol for all that is variable, in distinction to climate, which is the steady, average condition of things—is really due to the sun's variation.

This seems a bold claim. We shall see presently how he supports the claim. What interests us still more is that he finds it possible to predict weather for days, weeks, and even a month in advance, just by using observations of the sun's radiation and its changes.

The astonishing feature about his results is that very small solar changes, even those of less than one-half of 1 per cent, in the sun's radiation are able to produce considerable changes in the weather. This seems at first rather preposterous. We think of night and day, with 100 per cent change from light to darkness, and of the great change of intensity of the sun's rays between summer and winter. Neither of these tremendous changes of solar radiation gives tremendous changes of temperature.

We must forego possible explanations of Clayton's paradox, merely remarking that a small pull of a pistol trigger can do great damage; and something analogous may be involved here.

The answer the Weather Bureau must make to the "trigger" suggestion is that the idea is idle speculation, at least until Doctor Abbot or Mr. Clayton gives some physical or observational evidence of the catalytic involved. Assuming that the solar output, considered as a whole, fluctuates from day to day by one-fourth of 1 per cent there is nothing in our present knowledge of the subject to cause us to expect that the effect on the weather would be other than at least approximately of the same order of magnitude.

We are particularly fortunate in being able to publish in this *REVIEW* an excellent paper on "Fluctuations in the Values of the Solar Constant" by Dr. C. Dorno. The writer is glad to learn Doctor Dorno's views because they very fully confirm the conclusions reached at the Weather Bureau from purely statistical analyses of the observations themselves.

There are several reasons for the failure of the Weather Bureau to call attention to the volcanic eruption in the southern Andes in December, 1921, as a possible cause of the decided change in solar constant after March of 1922. Our attention was confined exclusively to the search for statistical evidence as to the *magnitude of the total short-period fluctuations* and what part of them one might be justified in ascribing to true solar origin. We have made clear in previous papers our belief that all of the now very small total fluctuations may well be due to instrumental and atmospheric influences. Moreover, and this is highly important, serious instrumental difficulties



developed at the Montezuma station in 1922 which the local observers were unable to overcome. All the observations from August of 1920 are provisional values only, and Dr. Abbot has stated that they will be entirely revised and republished in the near future.

Doctor Dorno is entirely correct in calling attention to the possibility that volcanic dust may have been one cause of the general lowering of the solar constant values during 1922. We hesitate to believe, however, that it is an adequate explanation of their continuance at this low level well into 1924. What effects are we to expect volcanic dust would produce upon properly derived values of the solar constant? Obviously none, if our measurements and their extrapolation to zero air mass are correctly done, because dust in our atmosphere can not change the intensity of solar radiation outside of the atmosphere. The principal effect of the Katmai dust was to cause large fluctuations between individual daily values as determined by the bolograph, accompanied by a lesser effect on the average measured intensity, which was, however, slightly lowered. Now, while we do find some increase in daily variability in 1922, which died away within a few months, the major feature is a conspicuous general lowering of intensity. During this period, nearly all observations were made with the pyranometer, a highly empirical instrument, whose behavior in contrast with that of the bolograph must be reckoned with in the interpretation of the apparent general lowering of intensity just mentioned. Doctor Abbot himself well says "a higher degree of accuracy in our solar measurements is needed."

In the light of experience, how is anyone to tell from the fluctuations of the derived values of the solar constant what were the true changes of intensity of the total radiation? Indeed, may it not be that we are approaching the point where it will be necessary to look beyond changes in intensity of the total radiation to changes in restricted spectral regions, if we would discover relations between solar radiation and the ever changing conditions of the earth's atmosphere? Doctor Dorno stresses the importance of the problem of the ultraviolet radiation, its nature, distribution in wave length, fluctuation in amount, as well as its possible meteorological effects. Doctor Abbot himself, in his 1925 Annual Report of the Astrophysical Observatory, page 104, says that "the importance of studies of the variation of the sun's output of ultraviolet rays grows upon our attention." Moreover, the results of Dr. Edison Pettit's work at Mount Wilson on the ultraviolet radiation of the sun seem to indicate that variations in intensity in this region of the spectrum are very great. (Carnegie Institution of Washington, Yearbook no. 24: 101-102.) We must not, however, overlook the serious difficulty encountered in attempting to allow correctly for the great effects the atmosphere exerts even under the most favorable conditions, upon the relatively feeble short and extreme ultraviolet radiations.

If the meteorological effects of these radiations are purely thermodynamic in character, we can not expect knowledge of their amount and fluctuation to aid materially in weather forecasting, because their total thermal intensity is but a small fraction of the whole. On the other hand, the physical phenomena associated with them are of great importance, justifying every possible effort to their full investigation. It may even be possible that some of the physical changes due to ultraviolet radiation in turn affect the thermodynamic behavior of the air in a way and to a degree that are of meteorological significance.

In conclusion, I wish to subscribe cordially to Doctor Dorno's comments upon the pyrliometer as a fundamental and basic instrument whose refinement to a higher order of accuracy is important.—C. F. Marvin.

The first part of Clayton's paper may be passed over without comment since, as Whipple points out (1), it relates to data which Doctor Abbot has rated as either "ancient" or "medieval" (2). His discussion of more recent measurements at Montezuma and Harqua Hala, which Doctor Abbot rates as "modern" (2) requires consideration, for the reason that his method of analysis brings him to a result that is not in accord with the conclusions to which I was lead by a different method of analysis (3). His effort to harmonize our differences seems to me to lead to quite unsatisfactory results.

In my own analysis I separated the 398 pairs of solar constant values obtained at Montezuma and Harqua Hala between October, 1920, and November, 1924, chronologically into three groups. The first group contained 99 pairs of values, the mean of which is 1.945; the second and third groups 106 and 193 values, respectively, the mean value within each of these groups being 1.922. The correlation coefficients for the different groups is as follows: first group,  $+0.341 \pm 0.060$ ; second,  $+0.18 \pm 0.063$ ; third,  $+0.17 \pm 0.045$ .

Clayton arranged the same pairs of observations so that the Montezuma values were separated by steps of 0.010 calorie, and for each class at Montezuma counted the frequency with which the simultaneous values occurred at Harqua Hala. In his analysis, however, he considered only the 53 pairs of values corresponding at Montezuma to grade 1.910-9, the 81 pairs corresponding to grade 1.920-9, the 66 pairs corresponding to grade 1.930-9, and the 44 pairs corresponding to grade 1.940-9, making 244 pairs in all, discarding all extremely high and extremely low values. He then computed the probable error of each of the four classes, finding it to be in each case approximately  $\pm 0.006$  calorie. Hence it is that our analyses lead to such different results. Clayton assumes that all of the 0.040 gram-calories in the range of the values in the four classes, except the probable error, is due to solar variability, and compares this  $0.040 \pm 0.006$  with the solar variability I obtain through the squares of my correlation coefficients, namely, 0.014 for the period October, 1920, to March, 1922, inclusive, and 0.003 for the period April, 1922, to November, 1924.

Attention is drawn to the fact that Clayton presented no correlation coefficients in this connection. Furthermore, his method of analysis includes all the secular variation in the solar constant values between October, 1920, and November, 1924, while my method excludes the difference between the mean values of the two periods named above, or 0.023 calorie out of the 0.040 calorie claimed by Clayton. In my paper I referred to the fact that a higher value of the correlation coefficient between the two stations would have been found had I included all the observations in one group. My object, however, was to determine the correlation coefficient between day-to-day values of the solar constant at the two stations, excluding, as far as possible, secular variations.

With reference to the significance of correlation coefficients I quoted authority (4) which, so far as I am aware, no one has yet questioned.

The remainder of Clayton's paper is devoted principally to a discussion of correlations between solar constant values and sun spots and weather changes, with reference

to which we may again quote Whipple as follows (1): "There is no attempt to show that the results are not attributable to chance, and, indeed, the general run of the graphs is in accordance with the hypothesis that they are."

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—H. H. Kimball.

In the investigation of any problem by the method of correlation, the real work does not begin until after the coefficient has been computed and its probable error determined. Even after everything possible has been done to insure against errors due to the nature of the data used, possible nonlinear regression, etc., there still remain numerous considerations which must be taken into account in addition to the mere face value of the coefficient; the difficulties in the way of arriving at conclusions which can be trusted, and the pitfalls awaiting, are numerous.

The problem quoted by Clayton on page 524, e. g., illustrates the following particular points: The correlation coefficient by itself is not an index to physical cause and effect, but merely an index of concomitant variations, however these may be brought about (the true measure of the degree of this relationship is the square of

the coefficient). Where we know a relationship must exist, the computation of the coefficient and the derivation of the regression "equation" serve the purpose of providing a quantitative expression of the relation, from which more or less useful predictions may be made; but in any case, while revealing to what extent fluctuations in one quantity are accompanied by proportionate fluctuations in another, the coefficient throws no light on the causal mechanism connecting the two.

The gross coefficient may result from the action of a third influence affecting each of the two variables correlated; or the mechanism may be of a much more complicated character. In Clayton's example, fluctuations of run-off did not accompany fluctuations in rainfall over the watershed, and the zero correlation reflected this fact; and as long as knowledge was confined to these two things, rainfall could not be used to predict run-off—these two variables are mathematically independent. So, if a large coefficient had been found, it would not have proved rainfall and run-off to be causally connected (though, in this case, considerations external to statistics would have suggested this as the common sense interpretation); but nevertheless, a knowledge of one would have enabled calculations of the other to be made, since they would vary together, for some reason or other. The tracing of relations of cause and effect, and the interpretation of gross coefficients, as well as the improvement of the regression equations, involves the computation in many cases, of net (partial) and total coefficients also.

Caution must always be exercised in applying the customary formulas and criteria to small samples, for they do not then always hold. Attention should also be invited to Walker's discussion of the criteria for the reality of correlation coefficients, *Mem. Ind. Met'l Dept.*, vol. 21, pt. ix, pp. 13-15, 1914.—*Edgar W. Woolard.*

## MONTHLY PRESSURE VARIATIONS IN THE NORTHERN HEMISPHERE AND SEASONAL WEATHER FORECASTING

By ALFRED J. HENRY

#### SYNOPSIS

The variations of monthly mean pressure for stations in the Northern Hemisphere, as published in *Reseau Mondial* for the eight years 1910-1917 were studied with a view of determining the frequency, geographic extent, and distribution in latitude and longitude of the pressure anomalies in the Northern Hemisphere for that period. The isanomalies for 67 months out of the 96 that were available were charted and studied. Many of these were featureless in the sense that the amplitude of the anomaly was small and frequently in an opposite sense in closely adjacent regions. In about 10 per cent of the cases considered the anomalies were pronounced both as to amplitude and extent of area involved. These are described in some detail, and the relation of the anomalies to current and subsequent weather in contiguous areas is discussed.

The paper closes with a brief review of the method of seasonal forecasting now practiced in India and tentative suggestions are given looking to the development of a method of seasonal forecasting for the United States.

*Variations from normal pressure.*—The air pressure at any given place is conditioned by several separate and distinct causes, viz (a) the intensity of incoming and outgoing radiation of which the incoming solar radiation is by far the most important; strong outward radiation from the atmosphere and the earth causes the air temperature to fall, the air mass to contract, sink, and thus the opportunity for the inflow of fresh accretions of air aloft and a raise in pressure is brought about; (b) the rotation of the earth on its axis modifies the speed and direction of air motion, causes it to be heaped up in places and set in swift motion at others whereby the pressure is elevated at the one and lowered at the other. The third or (c) class of pressure variations which form

the subject of this paper are due to a combination of the two causes above enumerated, in combination with those associated with the origin and movement of cyclones and anticyclones.

In general, monthly mean pressure for any given place will depart, more or less, from the normal in proportion to the frequency of cyclonic and anticyclonic systems experienced at the given place.

*Class (c) variations.*—In their simplest form these variations are experienced in the paths of areas of low pressure (cyclonic systems), the amplitude being greatest at and near the center and diminishing thence in all directions. It is perhaps needless to say that pressure falls with the approach of a cyclone and rises approximately as the center of the disturbance crosses the meridian of the observing station. If then more than the normal number of cyclones for the season pass over or near to the station the monthly mean pressure will, as a rule, be less than normal and the magnitude of the departure will be an index of the frequency of cyclonic systems passing over or near the station. Likewise a large number of anticyclonic systems passing over a station or lingering over it an unusually long time will result in a positive departure from the normal. Small departures either above or below normal are, as a rule of little significance.

*Amplitude of the variations.*—It is a matter of common knowledge that the amplitude of the variations under discussion increases with the latitude and reach a maxi-